Gallo, Robert 1995 B

Dr. Robert Gallo Oral History 1995 D

Download the PDF: Gallo_Robert_Oral_History_1995_D (PDF 157 kB)

Interview of Dr. Robert Gallo
Viruses Cancer Program
April 4, 1995

Interviewer:
Dr. Carl G. Baker, former Director of the National Cancer Institute.

Baker: Bob, we're pleased to have this opportunity to chat with you. And you got the questions that I put together? All are not always necessarily applied directly to you. But it would be helpful from the historical standpoint to have your views on some of the key events and the people involved. But first, could you give us a quick rundown of your background?

Gallo: I came to the Cancer Institute from the University of Chicago in 1965. My background was internal medicine at Chicago as an intern and resident. I was a medical student at Jefferson in Philadelphia before that. We won't go further than that. In '65 I came to NCI as a Clinical Associate. I found myself where I didn't want to be, on the childhood leukemia wards, but that ended up forming a lot of my subsequent work and career because, once allowed to go into the laboratory, I began working with white blood cells, their growth, differentiation--cell biology in other words--and at the same time began wondering about the mechanism of leukemogenesis, whether there was one or many, and in time I was drawn to the study of viruses, especially retroviruses, from people within this Institute whose reading and lectures influenced me that so many leukemia's of animals involved a virus, and especially a retrovirus, that perhaps at least part of my work ought to be in search of some in man. Then I formed, by the 1970s, at least, a third to half of my laboratory efforts were in this direction. It couldn't be 100 percent or I would never have publications. I got to know people in the Virus Cancer Program. I got to know you. I got to know John Moloney and Dick Rauscher on a scientific and social plane, and stayed close to that program, although I was in, at that time, the Division of Cancer Treatment. Many years ago I moved to where I should be, Etiology, but at that time in Cancer Treatment the Virus Cancer Program insisted on giving me some extra support so that we could be part of their program in a formal way. John Moloney arranged that, and I stayed close to that program throughout the last 10 years of John's career.

Baker: Well, we are interested in getting viewpoints from those who were not directly in the program, as well as those who were in it. You had a personal problem in your family that motivated you in this direction?

Gallo: Yes. I think it's hard for all of us to say for sure what led to decision-making, but it's impossible for me to avoid a component that was emotionally based, and that was the death of my sister when I was 12-13 years of age. Her illness started at 5. She died of acute lymphatic leukemia at the Dana Farber Center and that brought me into contact with academic physicians for the first time, but I also saw both the lack of treatment and the horror of that disease in that period of time. So, from that age 12-13 experience, then, coming back to NCI and ending up on childhood leukemia wards was a shock. It was not what I was planning. But it was a second jolt and, I think, really made me face the issue squarely that maybe that's what I'd like to work on--leukemia.

Baker: Well, you probably had the same feeling as I, that our inadequacies of therapy and prevention necessitated research as the only way to really do something about that?

Gallo: Yes.

Baker: Okay. I'll just get your views of the people who come to mind who are some of the pioneers in the Viruses Cancer area. And we're talking about the period from 1950 to 1980. As you know, in 1950, viruses in cancer work were looked down upon as not being realistic, and there were a few pioneers that kept the flame alive.

Gallo: Yes. I personally had involvement only from the year--let's say--1968-69 onwards, and especially 1970 and onwards, and a close connection to the program here roughly from 1973 and onwards. But, obviously the literature and discussions with people made me aware of things going back to the '50s. Well, certainly not directly did the avian people influence me, like Rous, or things like that, but of course they were important names. I won't go into those names that other people have documented. Obviously he got a Nobel Prize for it, so I won't talk further about it. But for me the mammalian leukemia-causing retroviruses affected me a lot, and immediately I have to mention a number of people who had viruses, at that time, named after them--we don't do that anymore, but a lot of times then they did--Ludwig Gross being the first, for the mouse leukemia virus. I didn't know Bittner and the MMTV story very well, so I have to stay with the leukemia viruses, Dick Rauscher, John Moloney, the sarcoma-causing viruses, Werner Kirsten, Harvey, the people who helped run the program--you--Carl Baker, Dick Rauscher again and John Moloney again, Bob Manaker in an often unselfish manner and patient manner, Ray Bryan, who I didn't know well but heard a lot about and I think he was one of the leaders. But he was kind of leaving the scene as I was entering, so I didn't get to know him well on a personal basis.

Baker: Did you get to know Sarah Stewart?

Gallo: Yes. I did know Sarah Stewart and I knew Eddy. So I knew both of them, but not well, but enough to, you know, have the fortune to have some interactions and conversations with them. And of course other people in that period, I said Werner Kirsten already, that had isolates and things that helped the field move in the mouse leukemia viruses, and here at NCI, Bob Huebner. Hearing him lecture was enormously stimulating for me. When I moved into this Building 37 here, he was a few floors beneath me, so we'd often get into debates in the corridors. He was pushing the endogenous retroviruses. Strangely enough, I had the audacity to argue with the person that I probably learned most of retro-virology from listening to talks, but I liked to argue why I thought the exogenous ones were going to be more important, and he would argue, "Do you think cancer is going to be catching?" And I'd say, "No, just some, in a complicated way, like Ludwig Gross talks about." And we used to really get into arguments.

Baker: Oh yes. He was a very stimulating fellow.

Gallo: And George Todaro. He was also, for a while, in this building. And then from outside the area of Bethesda: I mentioned Kirsten, I mentioned Harvey and Charlotte Friend, staying with retroviruses, but there was also another big influence, and it was a few people who were involved in this program but not so intimately. One was Bill Jarrett that affected me a lot, the cat leukemia story, from Scotland. He's still alive, sailing and not working in the lab anymore. But Bill's discovery of feline leukemia virus and showing it caused leukemia not just in a laboratory setting but in the field had a powerful influence on me, despite the fact that there was a hell of a lot of virus, easy to find, and that you'd say if humans were like that we would have found a virus long ago, you could easily still wonder what if a little virus could also do it, only it would be a little less frequently. And this was a guy that, kind of more than anyone perhaps, along with maybe Max Essex, stimulated me to stay in that kind of thing as opposed to the endogenous retroviruses, the infectious ones. And then there were the people in the bovine system that this Institute and this Program supported when I don't think anybody else on earth would have supported them. They were at the University of Pennsylvania at the time, Marshak, I guess, and a few other people-- Ferrer--but the bovine leukemia virus system was another key catalyst for me to stay involved because here was a viral-induced leukemia in which there was hardly any virus to find. It was very difficult to demonstrate it. So that took away the argument that it's easy to find a retrovirus when it causes a disease, so why do you need special techniques, an argument that was being used against me. Naturally, we had outside proximity to the Program, with the Temin and Baltimore discoveries of reverse transcriptase, which certainly made life a lot easier in looking for human retroviruses. And then came those wonderful meetings at Hershey where I really saw scientific interchange aggressively and openly more than I had ever seen anywhere else before, and I got to know an enormous number of virologists outside my own limited interest. So, the influences of people like Fred Rapp--

Baker: You're still having your annual meeting which, I wonder, is bases on the experiences you had at these earlier virology meetings? You saw the value of them?

Gallo: Yes.

Baker: Do you relate this to the Program itself, the Viruses Cancer Program? Are you talking about the meetings sponsored by them, or are you talking about other meetings?

Gallo: No. I was talking about the Virus Cancer Program meetings at Hershey, Pennsylvania. Exactly that.

Baker: Because communication, I think, was another outcome of this program.

Gallo: Absolutely.

Baker: And across narrow discipline lines too.

Gallo: Yes. I think absolutely. And I think also they had more than one kind of virus. I have that in my annual lab meeting, more than one kind of virus--even the ones I don't work on--just to have a few overviews that keeps me abreast reasonably. Sure. At that program I saw all the contract aspects. I learned of the availability of the reagents which were of immense value to us. Maybe you don't even fully know that when we published the first human retrovirus in 1980--really we isolated it in late '78--but before we published we had to be 100 percent sure it wasn't an animal virus contaminant, and I would say 80 percent--90--of the reagents we used in those first few papers came from the Virus Cancer Program. The isolation became possible from a few characteristics. One was our stubborn perseverance, which you might call foolishness. The second was our ability to grow T cells, which was not from the Virus Cancer Program but our accidental discovery of interleukin-2. And thirdly, were the rigorous interactions I had in the Virus Cancer Program where you'd damned well better know that the primary cells are infected, not just a cell line, that you'd damned well prove it isn't an animal virus contamination, and here is how you do it, and here are a host of available reagents to do so. And we used them.

Baker: You may recall--or maybe you missed this phase--that there was consideration that industry could not make virus preparations and antibody preparations of sufficient quality; that only the academic people could do that. And I had fun with the polio virologists by telling them that I admired the fact that they exchanged samples to check quality, but they had such a small amount that by the time they exchanged samples with each other they didn't have any left to work with. And Moloney came in one day, after Pfizer had produced large quantities of Moloney virus, and he was all excited because he said, "You know, this stuff, they've got buckets of it and it's just as good as anything we've ever produced."

Gallo: Joe Beard. Remember Joe Beard?

Baker: Oh, yes.

Gallo: We used so much of that avian myeloblastosis virus (AMV) material that he was making for investigation of reverse transcriptase and getting experience with it, but the experience was invaluable for what we did with human specimens later.But it is an exact repeated example of situations where you can't be too much out of your time or you don't get acceptance of credibility. Now, why do I say this? The Virus Cancer Program was using things like contracts and purchased this, that or the other, and, as you say, if it wasn't actually taboo, it was at least frowned upon in the ultra-academic circles. Right? Well, I remember getting attacked privately and semi-privately for relying on some aspects of data related to a contract. And I thought to myself, "You know, everything has got to be pure and double-checked by the individual accordingly in academia, and so you begin to ask, 'Well, what about your water? What about your isotopes from New England Nuclear?' You accept that because it's traditional. You accept your mice, that they are mice and that they're not carrying this, that or the other, that they're not filled with tumors already. You accept your radiolabeled thymidine, that it's not radiolabeled something else. We all rely on others."

Baker: We never did in the Lab of Biochemistry, in the earlier days.

Gallo: You check a lot of things.

Baker: You can't check everything.

Gallo: You can't check everything. But now look at 1995. The approaches by molecular biologists today, many of whom are working on disease, are dependent on obtaining reagents from all over the place--kits from this one, kits commercially available from here, contract available kits from there--for assaying this, that, or the other.

Baker: It's great. I wish I were younger so I could play in this game.

Gallo: Yes. Sure. But if you turn the clock back to '60 or '65, this game was not allowed to be played, and you started to make it that way and people were reacting who weren't part of the team. I think that was one of the problems of the Virus Cancer Program at the time. It was a little ahead of its time in that sense. The legacy of the reagents that the Program made should never be under-estimated. The viruses we had to work with, the direct lead to David Baltimore being able to find reverse transcriptase--maybe not in Temin's case--but the reverse transcriptase becoming a reagent that was also useful ultimately in gene cloning, those reagents came right out of that Program.

Baker: The next question deals with the key steps from the administrative or management decision-making. Again, it's just a matter of your impression of who comes to mind as key individuals. You already mentioned some.

Gallo: As I said already, my involvement with the Virus Cancer Program began around 1969 and in a more major way, maybe around '72; so, for personal recollections I can only date it from roughly the early '70s and, those who come to mind are you, Dick Rauscher, John Moloney, Bob Manaker, and a little bit Bryan, though he was completing his career, and I wouldn't call him administrative, but he was in every administrative lap. I would say Bob Huebner in some way or another was catalyzing (or de-catalyzing sometimes) a lot of the play at that time.

Baker: How about higher up organizationally?

Gallo: Higher up organizationally? You mean going to the White House?

Baker: And a little bit earlier. I'm really fishing for Ken Endicott because Ken Endicott made a key decision to go to Congress and ask for--

Gallo: I can't give you that. I came here when he was Director, and I only knew he had a nice reputation as Director. But I came in '65, and Endicott was soon gone after I came, and I don't know the relations then. You have to document that.

Baker: Well, we'll cover that elsewhere.

Gallo: I cannot document it that far back.

Baker: I didn't know whether you had had a chance to know him. He was a wonderful administrator and a great fellow to work for.

Gallo: The people I remember the best are the people I just told you about, and their helpers, for example, Jack Gruber, who is still here. Lou Sibal did a lot with Moloney in those days. All those people sure did a lot for us. Tim O'Conner. Remember Tim O'Conner?

Baker: Oh, yes. Sure.

Gallo: Tim O'Conner was John's left arm, if not his right arm, and was doing a lot of the interactions with the contracts, a lot of facilitating.

Baker: And part of the quality control, I think, stems from his insistence on good quality testing.

Gallo: Yes. He was a valuable asset to that program. He was full of energy and enthusiasm, sometimes too much of both, and he certainly helped me a lot. Tim O'Conner I owe thanks to, as to everyone else whom I mentioned.

Baker: Well, the third question is kind of a personal one on what do you consider your participation's main effects, if any? Your participation came more in the later part of the history here.

Gallo: It's the later part of the history of it. Yes. It's ironic because it was as the Virus Cancer Program was starting to be being killed when its fruits were coming to be obvious. In terms of my contribution, let us say, as the program was about being killed we were discovering the first and, soon after, the second human retrovirus. And shortly after that came the AIDS epidemic. All the background that the Virus Cancer Program did to make us be able to move rapidly was laid out by the Virus Cancer Program in many respects. So, if I think of my own contributions, it's ironic that they were happening as the Program was being compressed against the wall.

Furthermore, it was about that time--the very time we were discovering the first human retrovirus--that there was beginning of acceptance of the role of Epstein-Barr virus in human tumors. EBV was known before. It had its linkage a little bit to Burkitt's lymphoma somewhat before. But the data were not becoming acceptable until around that very period. It's also very soon thereafter that some papillomaviruses were beginning to be linked to cervical cancer. All these later events happened as the Virus Cancer Program was getting killed. In addition, it was becoming evident that a lot of American molecular biology was getting its stimulation from things that happened in the Virus Cancer Program. Not all of them, but a good segment of them. And that was just becoming apparent. So it's kind of ironic, as it was receiving its death knell, its success was just becoming evident.

Baker: Why do you think this happened?

Gallo: Well, I think it's, in some respects, a coincidence that everything--the evidence of it--was coming as it was being compressed. I think it was becoming compressed because of politics, money, and some people less successful who wanted to become more successful in terms of funding. I think people saw, that were in pure basic research, that cancer suddenly was important and was going to get funded and then a subject that they formerly wouldn't get involved in or dirty their hands with, in some peoples' minds, was getting to be better science and certainly a lot better money than they were accustomed to and that maybe they should be involved in it, and let's rethink about this and reorganize, because look at that money being spent over there. Did it really pay off? And probably when they began the assault it was prior to more visible, tangible evidence of the success. When the assault comes, people begin to look and say, "Hey, wait a minute. This did do A, B, C, and D," and then exactly when it was happening, presumably by chance--I have no other explanation for it--a number of other discoveries were coming out that showed, again, what the Virus Cancer Program was able to achieve.

Baker: So, if I hear you rightly, you're saying you helped demonstrate the effectiveness on the applied end of what was done earlier?

Gallo: Sure. Sure. It's obvious what the Virus Cancer Program did for me, and I think it's obvious what I did for its history--the "I" is collective in this group--and, you know, clearly, without the Virus Cancer Program, we would not have been able to do what we did and, doing what we did clearly was one example of--one modest example--of the success of the Virus Cancer Program.

Baker: We move next to the advisory committees. Some people thought that the contracts, of course, were not reviewed as well as the grant applications.

Gallo: Nor should they be.

Baker: Well, not the same way. They should be reviewed as well, but not necessarily on the same basis.

Gallo: Yes.

Baker: But do you recall any of the key advisory people, either individuals or as committee members, which were very important?

Gallo: Sure. There were all kinds of people making input to the Virus Cancer Program. I think it drew upon a tremendous amount of American virology, even international virology, and of course I recall that. Almost anybody you can think of. But there were people that were closer to the Program, because of their own interest and need for the program. It's just the way nature works. Fred Rapp being a prime example, the late Fritz Deinhardt being another example. They were very much involved in the program. They were good virologists who also cared about natural viral disease, not just laboratory systems. And these were people that Moloney, you, Rauscher certainly needed, Manaker and so they were very close to the program. Jeff Sloan, when he was outside the NCI, was close to the program. Sol Spiegelman was close to the program. The program needed molecular biologists interested in cancer, interested in viruses, and Sol was there. Some other people were not there. The program had little choice but to draw on somebody of that stature. I mentioned Bill Jarrett. He was here many times giving input to the Virus Cancer Program from Glasgow, Scotland. But, I mean, I can't think of anybody who wasn't making some input.

Baker: In formal committees for evaluation and approval, or not approval, I'm sure you participated in some of those committee meetings?

Gallo: I, myself, participated as an *ad hoc* committee member. I was on one committee for a while, I think for one or two years, and so I participated in some of those discussions.

Baker: Some of these were internal, and some involved external scientists.

Gallo: Usually, at the meetings I attended--

Baker: There was usually a mixture.

Gallo: There was usually a mixture. Yes. There was usually a mixture.

Baker: It was only for the final decision on contracts that it went up to internal final review, and then that was a recommendation which the Director had for approval or disapproval.

Gallo: I think people expected, on the outside--maybe they didn't expect it--they knew of their vulnerability. If you're trying to move money away from contracts towards yourself, or towards science in general, the one way to do it is to compare the quality of the science and, of course, contracts are open to be criticized for quality of science relative to a grant because a contract has to do something practical usually. You know, it doesn't take rigor to produce a virus, let us say, but a contract that produces AMV is a straightforward phenomenon. You're not putting in all kinds of basic research.

I think the contracts that got probably into the biggest criticism were those that supported what looked like basic research, like Sol Spiegelman's. Would Sol have been funded as well under a grant? Well, those are legitimate questions and legitimate arguments. That's the borderline of a contract, isn't it? It's our borderline. But yet Sol was able to direct an energy force and time towards very practical issues that I think you could argue that if the Virus Cancer Program didn't support you would have been criticized. A grant would not have supported his pushing reverse transcriptase assays to its success, and Sol did it to an excess and he was often wrong in retrospect, sadly wrong. But what if the Virus Cancer Program didn't support this? Taking reverse transcriptase by a molecular wizard, a molecular biologist wizard, who was willing to work on it fulltime and push it to the hilt and you said, "No, go get a grant," and he got a grant for his $100,000 dollars? I mean, Sol Spiegelman would have been up in arms. And you could argue that it would have been wrong not to support him.

Baker: Well, I, of course, wasn't concerned with supporting people one way or the other; I was concerned with looking at how you needed certain multidiscipline integration of concepts, and it wasn't simply practical resource production, so sometimes you needed research, not just production.

Gallo: You're right, 100 percent.

Baker: The grants system, which I support strongly for exploratory research, as strongly as anybody, did not deal with certain problems that required a mixture of disciplines, since most of the study sections are single discipline-oriented and most of the grant applications are. And so, while the planning grew out of developing a better basis for budget development, the systems networking, it was also a conceptual thing to deal with problems that involved integrating the efforts of very different people, different kinds of people.

Gallo: I understand what you're saying. I agree with you fully.

Baker: So it wasn't "either/or" as I saw it; it was a supplementation of the grants philosophy.

Gallo: But that's where you open your vulnerability, however, because you get into an argument of "who says?"

Baker: It's almost a religious phenomenon.

Gallo: Nobody is going to argue about resources, but people will argue that your choice was not the right choice. You're always vulnerable to it when you try to extend it to that.

Baker: But it seemed to me the grants system was not dealing with those multidiscipline kinds of things.

Gallo: I think there is no question about it. I mean, to this day we have problems--I think you see problems--in grant systems being able to deal with a disease, and that's why the success of the biotech companies--one reason for the success of so many biotech companies--or success may be the wrong word, but why there are so many people who want to make biotech companies. It's not just to make money, but it's to apply, in a practical way, coordinated different disciplines of research to get a job done bringing something from the basic research to the clinic.

Baker: Do any political figures come to mind? And here I use "politics" not only in the sense of the Congress but, shall we say, politicians in the science community.

Gallo: For me it was the ladies. Well, everybody knows their story over here, like Mary Lasker, and Deeda Blair. As a younger person in the Virus Cancer Program, I remember being invited--

[Speaker]: You don't want me to interrupt you for Governor Glendening?

Gallo: Who is calling?

Speaker: Governor Glendening.

Gallo: Governor Glendening? Oh boy, oh boy. He and I had better talk. Okay.

Speaker: Sorry.

Baker: We were talking about political figures and you mentioned Mary Lasker who is certainly one to be remembered.

Gallo: And Deeda Blair. Deeda Blair, I don't know how close she was to the Virus Cancer Program, but it had to be reasonably close because she cared a lot about viruses in cancer.

Baker: Well she, I noticed, went to a lot of meetings.

Gallo: Yes. But she also had a lot of dinner parties.

Baker: Oh, yes. I'm sure she followed the Mary Lasker system (which had similarities to the technique of Hollywood columnist Perle Mesta at her parties where people with various characteristics were brought together often to accomplish a specific task).

Gallo: Right. And I remember being invited prematurely to her house for dinner parties and finding myself at a table with a Congressman and a famous movie actress and usually a journalist. She would put these combinations together. And the scientist was ga-ga over the movie person, the politician was ga-ga over the reporter, and the reporter wanted to talk to the scientist about something, and it was an interesting circle.

Baker: This was the Mary Lasker technique which she used very effectively. I participated in some of those too.

Gallo: Yes. I remember that and let's call that politics. And I remember Senator Hatfield as a political person being very supportive at those dinner parties. That's how I met him on two occasions at dinner parties there. I didn't have the contact with…I didn't know Fogarty, for example, or some of the other people. I remember Mary Lasker bringing me down to meet, what's the Kentucky man, Thatcher?

Baker: Natcher.

Gallo: Natcher. And I remember talking to him about viruses in cancer and, you know, going probably over your head, over NIH's head, and Mary Lasker telling me to more or less, "Shut up and don't worry and just do what I tell you, and don't worry about your superiors. They'll be very happy you did this." I don't think you even know that. It was probably when you were Director. I'm trying to think of other people that I just may be forgetting about. I remember Ray Dutcher too. I forgot to mention his name. He died. He was a guy involved in a lot of the stuff with organizing meetings. I remember the Cherry Hill meeting. Ray Dutcher. Do you remember him?

Baker: Oh, yes.

Gallo: I don't know why he comes to my mind now, but John Moloney used to interact with him a lot, and he organized the Cherry Hill meetings. My first virus meeting was at Cherry Hill and was on the International Leukemia-Lymphoma Comparative Meeting, on the animal models, and it was really 90 percent virology. And that was the first time I saw some of these characters together.

Baker: Well, we've already talked on the value of the resources. A couple of things we didn't mention. Tissue culture itself really was pioneering almost up until the time of the program. Of course, Wilton Earle from NCI, George Guy from Hopkins and Ross Harrison from Yale were in on some tissue culture beginnings. Harry Eagle also made a big contribution by simplifying it and taking out the mysticism that was there before. But even so, we had a lot of tissue culture cell lines that weren't standardized. Some of them had contaminants; *Mycoplasma* particularly was a problem. Some of the cell lines weren't even what they supposedly were. They had the wrong labels. Some of them were animal cell lines when they were labeled human cells, and *vice versa*. So, again, the Program, I think, did a lot to clarify and standardize tissue culture cell lines in a very helpful way and made them available. Bob Stevenson was, of course, a key figure in the program, who later became Head of the American Type Culture Collection.

The animals. We weren't in a position to raise clean primates when the program started and a lot of developmental research money was put into animal husbandry and developing animals. So, if we had to produce healthy primates now in captivity, we know how to do it.

Gallo: Do you think the Virus Cancer Program catalyzed also the work with larger animals in this country in biology, disease biology?

Baker: Well, in the testing of the specimens from the humans some of the animals were larger primates, but also some were trying to develop smaller primates, so that was another development. Also cows were studied.

Gallo: Deinhardt had little primates, I think.

Baker: So partly that was done to look for a cheaper way than having to go to the chimpanzees which, of course, create all kinds of problems. But we didn't even have good data on the blood counts and distribution of cells in primates, particularly the larger animals, and the program did a lot in that direction. On tissue culture, we didn't even know how to freeze and thaw cells consistently when the program started and some developmental money was put into that which helped. Now we routinely, of course, freeze cells and thaw them, and nobody thinks anything about it.

Gallo: That's right. That's a fact.

Baker: Also, just a little side issue that the biohazards symbol was another outcome of the Program you probably didn't know about.

Gallo: No. I had no idea.

Baker: Nobody knows where that came from, but we had a contract which studied the various symbols for recognition and remembering what it stood for, and the one that was finally chosen was selected as the winner of a lot of psychological criteria which showed that people recognized it and remembered what it meant better than any other symbol. So that's another story that I think a lot of people don't know.

Gallo: No. I had no idea. No idea. I think another thing that's less tangible that you don't mention is that the Virus Cancer Program really catalyzed the meeting of people and that leads to the relaxation of your guard, open discussion, collaboration, sharing of reagents, an enormous amount of catalyzing of meeting of people, whether it be in John Moloney's office, or somewhere in this building (Building 37), or Building 31, or their annual meeting, or their reviews of this, that, or the other. There was so much of that would not have happened otherwise.

Baker: You're probably not too aware of the relative amounts of funding in virology cancer grants compared with the contract funding.

Gallo: No.

Baker: Well, we'll look into that, but that information is usually not appreciated by scientists in the lab, nor should it be necessarily.

Gallo: No. I have no idea.

Baker: If you could have changed anything, as you look back, would you see, first in your own case, and secondly do you see anything in the Virus Cancer Program itself that might have been done better?

Gallo: That's a very good question. I don't know why I didn't think about that before you asked me. My first inclination is to say maybe target a little more specifically some one or two diseases. Having thought that and reflected more on it, you see there would be, with what the facts are today, those cancers that we already know have a viral role are very unrelated--leukemia, lymphoma--sometimes unusual in fact, the indirect role of viruses like HIV and Kaposi and lymphoma, cervical cancer with papillomaviruses, skin cancer with papillomaviruses, and one could have focused on those particular cancers, and lymphomas in Africa, and nasopharyngeal carcinomas in China, liver cancer. And so I guess that could not be it. Maybe, for me, I would say, help with getting primary, fresh material. Early I wanted to use non-cell lines for tissue culture systems with primary human blood cells because there was a great deal of agonizing at getting such specimens in proper shape. Forming the relationship with the clinicians was very difficult. Getting prioritization, getting enough of the material, this was a real hell of a problem. Now, was the Virus Cancer Program trying to deal with it? Yes. At the time I probably wasn't nearly first priority. As my priority went up, things became better and I also had independent contacts with clinics. But I think, as opposed to permanent cell lines, I would have said that the focus on primary blood specimens could have been a little bit better. Secondly, and lastly, scientifically, I believe there was an over-emphasis on the endogenous viruses. This doesn't mean I think Bob Huebner was a negative force. He was the most positive scientific force in the whole Virus Cancer Program. But I think Bob became captivated by these concepts that he thought had to be right, the endogenous viruses.

Baker: I'll say one thing for him. Once the data showed his ideas were wrong, he dropped them and went someplace else.

Gallo: Like he never said it. No question. That's a lesson to be learned.

Baker: And a lot of people, I think, don't appreciate that.

Gallo: It's a lesson to be learned. But I just mean that because of his ideas, the force of his personality, his innovativeness, and his being there before somebody else, he had a little more persuasion for the endogenous viruses than I think was appropriate in retrospect.

Baker: I always refer to him as my General Patton. He was a dynamic leader, over-enthusiastic, driving hard, but you couldn't let him have all the resources.

Gallo I've been called that too when somebody was trying to say something nice, but I don't compete with Huebner. That's for sure. I've also been called, in *The Philadelphia Inquirer*, the Alvin Barkley of Science.

Baker: I don't know what that means.

Gallo: The basketball player.

Baker: Oh, that Barkley? I thought you were talking about Alben Barkley, the Vice President.

Gallo: No, no. Maybe I gave the wrong first name. Barkley. Charles Barkley.

Baker: Yes. Alben Barkley was the Vice President. I wondered how that fit.

Gallo: I don't follow basketball enough, like I should. Charles Barkley. In other words, you know, once in a while you can use your elbow on the floor. Yes. I think Bill Jarrett made that point to me. Bill Jarrett was right. There should not have been giving in to Bob Huebner's ideas so easily among some of those running the Program. It wasn't at your level. It was the next lower level. Maybe John was very much influenced by Bob and there was more of a focus on these endogenous viruses for a while than the logic merited. It lasted too long. Some percentage of it lasted too long. And that's about it. I can't give you anything other than that. The development of the reagent programs, all of these, were pioneering things. I mean they are things we imitate today. Fauci imitates everything that is required on the basis of previous Natiomal Cancer Institute activities. Broder used to like to brag, "He gets it all from the Cancer Institute." But where did the Cancer Institute get it from? The Virus Cancer Program. These reagents lists and all that. You know, the resource catalogs? They never had anything like that in NIAID. And NCI didn't have them until the Virus Cancer Program existed.

Baker: The question of political climate in the public's viewing of science--and I'm putting this very broadly, not just the cancer field, but science in general--and the understanding of science by the ordinary public, do you think this is better today, or worse, or the same, as it was in 1970?

Gallo: No. I think it's substantively worse. I think it's, no question, worse when you analyze it carefully. At first glance it appears better because there is more information by more people. Really what's happened is that there is much more superficial information by many more people--slogans, statements, loose ideas--and the old statement, "A little dumb knowledge is dangerous" is very apt here. And increasingly people seem--you know this is a hackneyed expression, but I think it's true--are reading less. They're reading less, more television, more slogans. They want more information, but they want it quicker, easier, and more comfortably. They don't want to stress themselves for it. So, as a consequence, you've got millions of people running around with half-baked notions. I think this doesn't help. Secondly, I believe the ability to manipulate genes, which causes some people concern, the rise in biotech companies, which gets scientists into business, hence patents, money, lawyers, publicity, those things together have not helped. You know, the fear of genetic manipulation and the lack of true understanding of it combine that with more activists in modern society--not just in AIDS--making more pressures, combined with the need for Americans to know every time a person belches, so that, you know, George Washington today would not be an icon. I mean, he must have done something wrong. He must have had a tooth that was a little yellow at the edge. I mean, they examine and reexamine you so that there is no such thing as anybody that's a hero, let alone--forget a hero--but anybody that's particularly nice. So, you only hear the negatives, and the negatives are exaggerated so, of course, like almost anything, I feel that science has fallen in peoples' eyes. But I think we are even more especially in the negative because of the reasons that I said.

Baker: The funding probably isn't going to reduce much in the cancer field because of the great fear, justifiably I think, of cancer. So, in a sense, the cancer area is a little better off on the funding, I think, than other areas.

Gallo: I suppose that's true. I hope it stays true. I hope AIDS will stay funded too since I work in both areas.

Baker: Well, not just for that reason, I'm sure.

Gallo: It's a good one though. That's a natural human instinct, isn't it? I know I'm natural, Carl.

Baker: A minute ago, on the question of how things might have been done differently, one of the persons I interviewed thought we might have paid more attention to public relations and we would have been better off with the Zinder Committee. I'm not sure that's so, but maybe we didn't pay enough attention to that part of it.

Gallo: If that person, by "public relations," meant private relations, he's right. I think you know what I mean.

Baker: I'll pass on that one.

Gallo: It means this. Cajoling and, you know, petting a few people. I don't think publicity would have done any good. No. I disagree with that. I don't think having public relations in publicity way would have done much good, because you didn't have a disease you could say you solved. You see, you couldn't say that, and so that wouldn't affect the public, so I don't believe public relations would have done much; it would only antagonize these guys. But I meant what I said, private relations, yes.

Baker: The question…I guess you've already answered the idea that the Viruses Cancer Program did help lay foundations for both molecular biology and for biotechnology.

Gallo: There is no question about that. You should document that as much as possible and detail it as much as possible, even though it may be a little boring and a little difficult, because that is a legacy that many people won't know about, some will try to take away, but the reality is it cannot be denied.

Baker: And lastly, do you have any additional comments that you'd like to make?

Gallo: Yes. I miss it.

Baker: Well, that's very nice.

Gallo: Well, it's true. I miss it. I miss the people.

Baker: Well, I'll throw you a compliment back. In your lectures you impart more information in the time allotted of any speaker I've ever heard.

Gallo: Thank you very much.

Baker: And I always learn, of course and your enthusiasm comes through.

Gallo: Well, I got that from somewhere. But I meant what I said. I miss the people. I miss the gatherings, I miss the interplay, and I miss the debates, I miss the arguments, I miss the occasional fights.

Baker: You mean you don't have them now?

Gallo: Not with the same camaraderie and friendship.

Baker: Really?

Gallo: I don't think so. I don't have that. There isn't that nest in the business I'm in right now of this kind of--at least I don't have the same feeling--for the individuality of the characters also. There was much more individuality of people then. Or am I getting old? I don't know. I mean, it seems to me that people are much more homogenized now, the younger people, more technocratic, not as interesting, and not as individualistic. Competent, maybe even more competent, but not as biologically well trained, not as conceptually well trained, and certainly, to me, not as interesting.

Baker: What do you think accounts for the younger people not being as well trained, you said?

Gallo: Oh, no. I said not "conceptually" as well trained, not conceptually as good, but better in technology. I think the reason is they have less personal contact with characters themselves. We're having less interesting characters, less individuality. There is more fear of getting in trouble. There is more homogenization of the world, let alone of ourselves. More rules, regulations, more technology, more new technologies to learn, so that you don't get as much conceptual discussion and you don't have as many characters.

Baker: Well, one reason for wanting to write this history, of course, is because conceptually you need to have a feel for how those ideas developed, I believe, to understand them more deeply, and so history is not getting its fair shake here, I think, among the training of scientists.

Gallo: This is a very good point and one that I often make when I go to places too. It was shocking to me the first time it happened, and I use it now to bring it up when I go to places to talk with students. You know, sometimes I get into that situation where they ask me to spend time with students, and I usually have a good experience. But one of the things in the graduate students in molecular biology I asked in this last 5, 6, 7 years, "Did you ever hear of Sol Spiegelman?" and nobody has ever heard of Sol Spiegelman. That will make you think about other things, but if you're looking for recognition, *sic transit Gloria*. Well, so, I say it's interesting. I mean, he practically invented molecular hybridization and a few other things, major molecular biological things, but the phenomenon that he was forgotten within a year of death, two years at the most, is really striking to me.

And then you get into what they know of history about people, colorfulness, the concepts of it, how things became the way they are, and they have no idea. There is nothing.

Baker: Well, you probably noticed it, at talks now, when young people get up to give a talk, they start out with, "First slide please." They don't give you any lead-in history at all. Of course, part of this is the intense competition based on publications. The young people feel like they have to devote their efforts to another paper, and they're under a lot of pressure, so--

Gallo: Technology.

Baker: So history is kind of a luxury. But it's not really, I believe, in a conceptual sense.

Gallo: No. That's where you learn how things happened. You learn how discoveries were made. You learn what's most significant.

Baker: And conceptually it's important if you're going to question so as to know what the basis for a concept was in the first place.

Gallo: I absolutely agree with that tremendously. I think that they really don't-- I almost think it should be required, that a history of the field you're in, should be required for you in that particular graduate program.

Baker: But a good mentor ought to be able to impart some of that without a formal course.

Gallo: Yes. Well, I don't know. There are mentors, and there are mentors, and there are mentors, and it depends on your luck. But I think if I do this thing at Maryland, or wherever I end up, one of the things we've been thinking about is human virology. I have Bill Blattner going with me, one of the excellent epidemiologists at NCI. He could cover the epidemiology of all the major viral diseases that we would want to be focusing on. This is a human virology department, we're thinking. Bob Redfield, who heads the Walter Reed Army Program in AIDS, will do the clinical. And my lab and I will do the laboratory side of some of the major viruses.

Baker: Gee, you're multi-disciplined, because you want to tackle disease on those fronts to get at it.

Gallo: I certainly do. We're going form an Institute of Human Virology with three components.

Baker: Well, on teaching, another problem is I think most science departments are not interested in teaching science to non-science majors; they're only interested in those students who will be going into science. And then they wonder why the public doesn't understand them more.

Gallo: I have sympathy for both sides. On this I'll play Carl Baker the diplomat. Well, you can understand it from both sides.

Baker: That's why I have been teaching some at Maryland, to try to teach science to non-science majors.

Gallo: Are you at College Park?

Baker: Yes. University College though. These people work all day. But they're motivated anyway.

Gallo: Yes. Sure.

Baker: But most of them only took the course because they had to have a credit in either science or math. So I took them from basic physics right on through to behavior and evolution and the genetic code and meteorology and oceanography and all in between. So, it was kind of fun.

Gallo: It must have been a lot of work too?

Baker: A bit. I had to go back and review physics I hadn't had for 40 years. I worked the problems through and I showed them how you can determine the mass of the earth comparing it with an apple and then you can throw out the apple in the long-run in the calculations and you don't even need the apple. And then I showed them, you know, how much thrust you had to have to get the satellites up, and things like that. It was kind of fun. Well, I worry at NIH that, it seems to me, the political decision-making has invaded a lot of areas that are really scientific matters. One reason I didn't like some of the new Cancer Act was it made the head of the NCI a presidential appointment, and the same with the director of NIH. I think it was better the old way, where you weren't tied to a particular party. And, of course, the Surgeon General's position is about shot.

Gallo: But you ought to wait until Clinton goes out before you change the rules because Varmus is really, you know, pushing for a compression of NIH. There is no doubt. And so I don't think Varmus is in love with the place he's director of. You know, from what I see, certain clinical and epidemiology work, even though he's an M.D., he's pushing them out. So this is not going to be the heyday of NIH anymore. And once you cut clinical and you cut epidemiology you could argue why do you need NIH. If you want a bunch of small labs with relatively young people, what do you need it for? You have that at any college, let alone a university.

Baker: Well, Tom Kennedy has written a paper which will appear in the *NIH Alumni News Letter* shortly, that's trying to argue why we ought to have the intramural strong, and he cites the record where he's comparing *The Citation Index* and other criteria to show that NIH is right up at the top on its contributions. And why should you change that? Well, I don't know. There are a lot of forces that are going to be interesting to see. But we certainly had a great time at NIH. We had some wonderful people, both in and out.

Gallo: Well, that's for sure. But I think this is an era that's coming to an end. I don't think there is the support for it anymore. I mean, people say, "Oh, no, Congress won't get rid of NIH because they want to have it to play with," but I don't agree with that, because the states see it to their advantage to put it down. The NIH created, I think, its probable downslide when it created all the other centers, AIDS, Cancer, Molecular Biology, Gene Therapy, Neurology. You have all these centers all over the nation now. And that's a good thing. But then you can start questioning, "We need money. Why do we need this guy anymore? Why do we need grandpa?" You know, it's an interesting thought and what about Varmus. Gajdusek believes that Varmus has a superplan. Here is Gajdusek, not me, talking. He said, "All the slots are going to the Human Genome." I said, "Yes, maybe you're right." He said, "Did you read what Varmus said before he came here?" I said, "No." He says, "That the Human Genome Project should be in something like IBM, not at a scientific center. So if you pile up all these slots there and you reduce epidemiology, reduce clinical epidemiology, what do you have less?" I said, "I don't know. What do you mean, less collaboration?" Gajdusek is much more political than I thought, and he said, "You have less of it to support. You don't have that ability." And he said, "You don't have much ability to support, and now you've got everything that you think should go to IBM in these slots over here, and one day you press a button and it's IBM." And he says, "And you've reduced NIH to that overnight." And it's an interesting thought, and it may be exactly what's happening.

Baker: Okay. Well, I certainly appreciate the time and your comments and frankness. It's always fun to talk with you.

Gallo: The same with me. Thank you very much.

Baker: Good luck to you on your new ventures. I hope you make the right decisions for you.

Gallo: I hope so too. I don't know. It's complicated. I'm talking to four places at one time, but I'm down to the wire now and I've got to make a decision.

Baker: As to which is best?

Gallo: Yes. But if it's Maryland, then you'll have to come over as visiting professor. I mean, I'd be in Baltimore if it's Maryland. So, it's either going to be in Philadelphia, Maryland, or Virginia, or in Charleston. Charleston is certainly the most attractive to live, but it's also the weakest medically. It is not a big town. You know, Baltimore has got the action.

Baker: How's the family?

Gallo: Good. Thank you. And you?

Baker: Fine. My wife retired, well it's been about two years now, and she's enjoying it no end. You knew I had part of my stomach out, I guess?

Gallo: Yes?

Baker: Oh, yes. I got three-fourths of my stomach out with stomach cancer.

Gallo: Oh, no. No. The last time we had a long talk was at O'Donnell's, do you remember, and you had gone through one hell already for something else. What was that? That wasn't cancer though.

Baker: Well, I had sepsis on top of the stomach cancer.

Gallo: When was the stomach cancer? How long ago?

Baker: About three years.

Gallo: Maybe it was that. I didn't think it was a cancer though. But I talked to you where you had…

Baker: Well, I also had sepsis, so they gave me all the gentamicin.

Gallo: Had you had anything before this?

Baker: Well, I had melanoma, but that wasn't much of a problem.

Gallo: That wasn't it. So this would be it. This was the only real significant thing since we've been out of touch?

Baker: Yes. I, of course, lost 27 pounds and--

Gallo: Oh, there's no doubt you've lost weight.

Baker: Yes. I'm doing pretty well, but I'm still unsteady when I walk because my inner ear nerve endings are shot. But I'm making out fine now. I'm playing golf.

Gallo: Your inner ear is shot? Why is that?

Baker: The gentamicin knocks out those nerve endings.

Gallo: Oh, gentamicin. Sure.

Baker: So, I'm playing golf, not much worse than I was before. I gave up skiing though. I learned to ski at age 57, when I lived in Switzerland.

Gallo: That's funny. I'm 57 actually just turned 58 last week.

End of Interview